Review of "Technical documentation to support development of minimum flows and levels for the Loxahatchee River and Estuary"
SFWMD Water Supply Division, July 15 2002 Draft
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Summary

The MFL proposed for the Loxahatchee River and Estuary is designed to provide adequate flow to the Northwest Fork of the River to protect the floodplain swamp community. Flow recommendations were obtained as follows: 1) a 2-d hydrodynamic model was developed that relates current flow conditions to salinity, 2) historic flows over the Lainhart Dam (1971-2000) were used in the model to hindcast daily average salinities at various places in the estuary, and the predicted salinity records were evaluated to determine both the frequency and duration of events when the water at each location was greater than various thresholds (e.g. 2 ppt), 3) a survey of the floodplain swamp community was conducted along the river, a subset of six trees were chosen as valued ecosystem components, and both the presence/absence of these trees along the river as well as their characteristics were used to identify healthy, stressed, and significantly harmed locations (at RM 10.2, 9.7, and 9.2, respectively), and finally 4) an MFL of 35 cfs at the Lainhart Dam was chosen (not to be exceeded for more than 20 d more frequently than once every 6 y), based on the model predictions of flow and salinity at the identified locations, with the goal of preventing damage or stress from occurring to the floodplain swamp community at RM 10.2 as well as preventing significant harm from occurring at RM 9.2. Additional information on soil salinity along the river, changes in vegetation over time, the relationship between flow and observed salinity, and estimates of consumptive use are also included in the document, but this information was not used directly in selecting the proposed MFL.

It is clear that the staff of the SFWMD has put a large amount of effort into the proposed MFL, and this is in many ways an improvement over the previous draft document. The report does an excellent job of addressing the comments provided in 2001, the literature review is improved, and the document is better organized. I think the shift away from cypress as an indicator is warranted, and the selected freshwater tree species provide a reasonable basis for discerning differences in the health of the floodplain community along the salinity gradient. However, there are some fundamental problems associated with the application and interpretation of the hydrodynamic model, and I do not think the document as it now stands adequately supports the proposed MFL. Below I review the major components of the proposed MFL as organized in Chapter 5.

Conclusions

Literature Review.

This is much improved over the previous version, in particular because there has been a clear effort to locate information on the salinity tolerances of cypress. However, the document would benefit from more information on the life history characteristics, functional roles, and salinity tolerances of the 6 chosen indicator species.

VEC Approach.

I'm not sure this is actually an application of the VEC approach. There is a complete list of resource functions and services provided in the document, but they are not tied very well to the floodplain swamp community. Instead, the trees that were identified are useful as indicators, rather than particularly "valued." The document indicates that these species were chosen because they occupy different ecological niches and have different functional roles, but this is not well documented. The species chosen are all relatively long-lived, and it seems like including some herbaceous species with shorter life spans is perhaps worth considering as they might provide faster response times and a better cross-section of the community.

Historical flow and salinity data

The historical flow data is presented as a very long table in Appendix D, without comment. One concern I have is whether these data were all corrected, based on the recalibration that occurred recently (this goes for Tables 23 and 24 and Figure 20 in the text as well). Although I understand that flows at G-92 are correlated with those over the Dam, they're not the same, are they? If they are, this should be stated. If not, the document would benefit from a presentation similar to that in Figure 19 of flow over the Dam since that is what is being regulated. Table 24 and Figure 20 are useful, but it would be instructive to see some summary data (e.g. different percentile flows) for the period from the reference year (1985, if that is selected) to the present.

The salinity data presented in the document are interesting. One suggestion is to recalculate the information in the Wild and Scenic segment of the river without station 63 to determine if average salinities have in fact increased over the past decade (as referred to on p. 102). This is an important point: elsewhere in the document the data suggest that flow has increased over the past decade and it would be very useful to know whether this change in flow has resulted in a measurable change in salinity or whether increased flow over the Dam has been offset by other changes in the watershed.

The salinity data presented in Appendix D were used to calibrate the hydrodynamic model, but the empirical relationships between salinity and flow were not used in any way in this document. I think these relationships are extremely useful (particularly those derived for current conditions, after the gaps had been closed) and might be appropriate as either a check on modeled salinity/flow relationships or as the basis for setting an MFL (see below). The original relationships, which were computed using Excel, are presented in figures D3-D6. These are very poor fits, and, in response to my comments on last year's document, they have been redone in SAS using variable flow-averaging periods (pages D11 – D22). The SAS fits are much improved over the ones done in Excel and could be very useful. Curiously, the SAS analysis is not referred to anywhere in the text, and SAS analyses were not performed for stations 66 and 67.

Aerial photography/GIS

This was a straightforward, complete analysis of vegetation types in the estuary over time. However, I find it worrisome that no major changes in vegetation cover were observed between 1985 and 1995. The footnote in table B-4 indicates that vegetation in a segment of the river below Trapper Nelson's was estimated from 1995 photographs. Could this substitution have perhaps led to the erroneous conclusion that things did not change in this area? Given the improvements in G-92 and the resultant increase in flow that occurred in 1989, was there a concurrent decrease in salinity (as mentioned above)? If there was an increase in salinity, wouldn't we expect to see a downstream shift in the indicator community? Perhaps this is the explanation for the field observations reported on p. 132 that suggests the location of the stressed area has moved downstream between 1985 and 1995? This needs to be explored. If there has been increased flow and decreased salinity, which in turn has led to a shift in tree distribution, that would be good evidence that the indicators are in fact appropriate. It might also mean, however, that the choice of 1985 as a reference year would result in managing towards a situation with less freshwater inflow than occurs now.

Finally, when evaluating shifts in vegetation it is worth keeping in mind that there are other factors that could account for changes in vegetation besides changes in hydrology.

River vegetation survey

The results of the vegetation survey show a clear gradient in the distribution of the 6 chosen indicator species in the floodplain community, and, although there is not technical information in place on the salinity tolerances of the various trees over the course of their life cycles, it serves as a useful starting point for the identification of healthy, stressed, and significantly harmed locations along the Northwest Fork of the River. Although these are judgment calls, the selected locations are supported by the data in terms of observed changes in the presence of the various species and by their measured characteristics (e.g. as we move downstream, fewer VEC species are represented and those that are there are smaller, with fewer seedlings and saplings). Given the fact that these trees used to occur further downstream, it is probable that salinity is an important factor that controls their distribution. One point to note is that the trends do not level off (e.g. as we move up to RM 10.6, trees are more abundant, larger, and have more seedlings and saplings). One wonders if another station further up-river would yield even more, in which case the selection of a representative healthy site might need to be re-visited.

Soil salinity samples

The observation that chloride shows a better gradient along the river than soil salinity is most likely due the fact that salinity has a much smaller dynamic range (it is constrained between 0 and 36). This makes it a less sensitive measurement, but I do not agree with the interpretation that this suggests salinity is not retained in the soil.

Hydrodynamic/salinity model

Although the 2-d model does an adequate job of matching long-term field salinity trends, the figures in Appendix E suggest some real discrepancies between observed and modeled salinity. This is acknowledged in Appendix P (p. P-4), where it states that salinity in the upper estuary is extremely sensitive to freshwater input and points out that the majority of the freshwater input was estimated from ratios (which are quite variable in reality but are fixed in the model). I understand new surface flow stations are addressing this, but without this information, and with another large estimate of inflow from groundwater (estimated as 40 cfs in a system where 35 cfs from the Dam is being proposed as the MFL), predicted salinities in the upper estuary are extremely suspect. The model may be a useful tool for exploring different management scenarios, but I am concerned about the over-reliance on model predictions of salinity as the basis of the proposed MFL.

It is instructive to compare the flows/salinities predicted by the model with those derived from the analysis presented in Appendix D: according to the model, the flow required to maintain a high tide salinity of 2 at RM 8.6 is 54 cfs (obtained from Table 7 on p. E-18), whereas an average bottom salinity of 2 ppt is correlated with a flow of 64 cfs (p. D21). At RM 7.7, the model flow is 89 cfs (again to maintain a salinity of 2). This matches the Excel fit quite well, but the prediction from the SAS relationship is approximately 140 cfs (p. D18). This suggests that **the model may underestimate the flow required to maintain salinities at their target levels and/or underestimate salinities at any point in the river, which would result in an inaccurate MFL**. If the intent is to link flow and salinity it would be more defensible (and simpler) to stick with the empirical relationships derived in Appendix D.

Even if the model were judged as the most appropriate tool for predicting salinity at different locations in the river under different flow conditions, it makes no sense to use a flow/salinity model calibrated with current data to predict 30 years worth of salinity. First, the document makes clear that there have been extensive changes in both the watershed and the estuary over that time period, such as dredging in the estuary, changes in land use resulting in changes in the amount of overland runoff and groundwater infiltration, and closing the "gaps" (which added 0.7 miles to the river). All of these changes could affect flow/salinity relationships, making historic salinity predictions based on current relationships less accurate. At the very least, some of the model predictions could be compared to historic salinity data (e.g. Appendix A describes studies by Chiu (1975), Hill (1977), Russell and McPherson (1974), and Law Environmental (1991), all of which collected salinity information).

Second, even if it could be demonstrated that the model can in fact be used to predict historic salinities, flow conditions have changed over the 30-year time period: The G-92 structure was not constructed until 1974, its capacity was increased in 1986 and additional culverts and operational criteria were added in 1987. In fact, the document states that flow over the Lainhart Dam averaged 52 cfs from 1977-1989 and increased to 86 cfs from 1990-2001, and that the occurrence of flows below 35 cfs decreased from 34% of the time to 25% of the time between the two time periods. This means that salinities at given locations in the river were very possibly greater before 1987 than they are today (this could be verified by comparing some of the field observations). Moreover, the reference point chosen by the SFWMD as the basis for establishing an MFL is 1985. It therefore does not make sense to look back to 1970.

All of the problems stated above mean that using a 30-year record to determine salinities (and deriving statistics about the average amount of time salinities at different sites are greater than a particular threshold) is not useful for understanding current conditions or setting MFLs. That said, the Ds/Db ratio is extremely interesting and looks like a useful approach for summarizing salinity data. Perhaps it could be used to characterize field salinity observations (e.g. between 1997 and 2000).

Vegetation/Salinity model

The MFL was chosen based on the model-predicted salinities at the locations identified in the vegetation surveys as healthy, stressed, and significantly harmed. To begin with, the goal of the MFL is not clear: if RM 9.2 has already been identified as an area that is experiencing significant harm (over what time frame?), then it makes no sense that the flow target has been chosen to prevent significant harm from occurring there (as stated on p. v and p. 149). The time frame is also not clear. On p. C-16 it suggests that long-term average salinity conditions since

1970 have led to the decline in freshwater vegetation, yet the analysis in Chapter 5 suggests that using those long-term averages is an appropriate basis for protecting the resource from further harm. Once the baseline condition gets sorted out (is it 1985? and has flow, salinity, or floodplain changed since that time?), this needs to be revisited.

If current vegetation at RM 10.2 is deemed healthy and the MFL goal is to protect it from harm, then what is required is to provide as much flow to RM 10.2 as it currently gets (i.e. the status quo). If this is the case, it would be much more straightforward to analyze the flow record over an appropriate period (e.g. since 1985, or perhaps since G-92 was improved or since the gaps were closed) and determine average flow (or a particular percentile flow, or the proportion of time that flow falls below a particular percentile). Interestingly, the report states that average flow over the Dam was 70 cfs from 1971-2001 (p. 160). In comparison, the model results presented in Table 40 suggest that 50 cfs is required to maintain average historic salinities of <0.15 at RM 10.2 This again suggests that the model is underestimating flow.

If the MFL goal is to provide enough freshwater so that the salinity regime currently experienced at RM 10.2 can be reproduced at a downstream location (e.g. RM 9.7 or 9.2), then it becomes necessary to understand the relationship between flow and salinity, and this is where the model comes in. However, even if the model were appropriate and could be used to predict salinities at these river locations, I find the logic here extremely convoluted. What is essentially happening is that a) the model begins with a relationship between salinity and flow, b) historic flow data are used to predict historic salinity, c) historic salinity data are used to determine Ds and Db, d) Ds and Db are related back to flow, when all that is really needed is the relationship between salinity and flow.

Moreover, when I followed the data in order to do a "reality check" on the model, things did not add up: Table 24 reports that flows of less than 35 cfs at the occurred 25% of the time at the Lainhart Dam between 1990 and 2001, and 35% of the time between 1971 and 1989 (for an average event duration of 15 or 24 d with a return frequency of approximately 2 mo). In Table 37 the model predicts that a flow of 35 cfs will result in a salinity of 2 ppt at RM 9.2 (the basis of the proposed MFL standard), and in Tables 35 and 36 we see that model-predicted salinities of 2 ppt occurred on average for 46 d every 6.8 mo, or 18% of the time at RM 9.2. I recognize that there is a response time built into the model and that we cannot expect a 1:1 correlation between flow and salinity, but these estimates of Ds (46 d), Db (6.8 mo), and % time over the threshold (18%) are very different than the flow observations (15-24 d, 2 mo, and 25-35%, respectively). Likewise, flows of 10 cfs occurred 7% of the time in the data presented for the dam (an average of 19 d every 9 mo). However, at 10 cfs the model predicts a salinity of 2 ppt at RM 10.2, which is estimated to have occurred only 1% of the time (an average of 22 d every 6 y, which is also used in the proposed MFL). Either I've misinterpreted these results or the model does a very poor job of estimating these parameters and should not be used to select an MFL.

I would suggest either working with the empirical relationships derived in Appendix D that relate flow to salinity or improving the model so that it does a better job of reproducing observed salinities. In either case, it seems like the historic salinity information is not relevant and the MFL can be set based on the current salinity regimes (e.g. it would be possible to determine what flows would be necessary to change salinity conditions at RM 9.2 such that they mimic what is currently observed at RM 10.2).

Finally, I'm not sure I understand why the emphasis is on 2 ppt. If these salinities are thought to occur very rarely (e.g. the 99th percentile), then flows could theoretically be maintained at the 98th percentile without violating the MFL. However, maintaining a salinity of 1.9 at RM 9.2 would surely cause damage to the vegetation even further upstream in the River. Is the target actually to maintain average flows such that average salinity at RM 9.2 will be what is currently experienced at RM 10.2?

Consumptive Use Permit Analysis

I do not have Appendix I, but it looks as if this is a complete review of consumptive use. If dry season impacts are 5 cfs, this could be important when flows get low.

Recommendations

I do not think the MFL should be adopted until the following points are addressed:

1. 1985 as the base year for this analysis should be carefully considered. Part of this decision should be based on a determination of whether a) flow conditions, b) salinity observations, or c) vegetation has actually changed in the river since 1985. (Another possibility would be to use 1997 as a base year (after the gaps were closed), as this would make the flow/salinity relationships more straightforward.) Whatever the base year, all analyses of average flow, salinity, and vegetation should date consistently to that year.
2. The MFL goal should be clearly stated. Is it designed to maintain current conditions at RM 10.2 (the status quo) or improve conditions at 9.2 such that the floodplain community at that site is similar to what now occurs at 10.2? It cannot be to protect RM 9.2 from significant harm, as stated in the document, since this is already occurring. If there is a difference between management goals and MFL targets, this should also be stated. However, selecting an MFL at the 99th percentile flow is not likely to meet the goal of protecting RM 10.2. Managing for the 90th percentile might be more appropriate.
3. The hydrodynamic model as it stands now is inadequate for providing accurate flow/salinity relationships. The model needs to be improved, or the relationships developed in Appendix D (using SAS) should be used for this purpose. Only relationships based on current salinity conditions (after the gaps were closed) should be used, and there should be no attempt to use historic salinities for this purpose.
4. If it makes more sense to determine the MFL in terms of salinity than flow, the analysis of Ds and Db should be done based on empirical observations of salinity at each site.

Other Comments:

1. I assume it was a policy decision to limit this MFL to the Northwest Fork of the Loxahatchee. The document is uneven in this regard, since so much information is presented on the other tributaries. However, it is informative and serves as an important reference for the whole Estuary.

2. Since there's no control over the flows in the other creeks in the Northwest Fork (and since they occur downstream of RM 9.2), maintaining the floodplain community at RM 9.2 may not

help the entire estuary. This means that it might be appropriate to add additional indicators in locations further downstream.

3. I applaud the District's efforts to incorporate an adaptive management component in this effort. The proposed work on monitoring tributary/creek flows, the groundwater investigations, continued salinity monitoring and vegetation sampling should all provide useful information that can work to improve the MFL criteria.

4. The document could benefit from some careful editing to reduce redundancies.

Specific comments:

p. 44 - Please clarify whether the information in Figure 10 (and the discussion of the figure) is a presentation of allocation or actual water use.

p. 64 – What are the units for the contour lines?

p. 80 – I think the reference to Tables 15 and 16 is supposed to be Tables 16 and 17.

p. 87 – The statement that the model fits the estimates presented in Appendix D needs to be reevaluated in light of the SAS-derived estimates.

p. 93 – Please add a sentence to #2 to give an indication that there's considerable variability in these proportions.

p. 115 – What is the reference point for the statement that major changes have occurred in vegetation downstream of RM 9.2?

p. 135 – All 6 plants chosen are freshwater species, so the last bullet before the summary needs to be modified.

The statement that a healthy floodplain community exists to RM 9.8 is not substantiated by the observations, since there is no data and RM 9.7 shows fewer, smaller trees as compared to RM 10.2

p. 142 – It would aid in the interpretation of Table 38 if somewhere in the document or Appendix the locations where each of the parameters for each species is considered to be in decline were identified.

p. 145 - Why don't the criteria used in the top of Table 40 match the observations reported for RM 10.2 in Tables 35 and 36. The observations indicate that salinity at 10.2 was greater than 1 for 30 d every 1.6 y (or 5% of the time), greater than 2 for 22 d every 5.9 y (or 1% of the time), and greater than 3 ppt for 13 d every 30 y (or 0.1% of the time), and the text states that the MFL was set not to exceed 2 for more than 20 d every 6 y, in keeping with these observations. However, the criteria developed in Table 40 are for salinities greater than 1 ppt for 40 d/y, 2 ppt for 30 d/4 y and 3 ppt for 20 d/10y, which represent 10%, 2%, and 0.5% of the time, respectively.

Presumably, this means that the flows reported in the table are greater than they should be if the goal is to match the observed flow regime at RM 10.2.

p. 148 - Does the statement about providing flows comparable to historic rates represent a management target as opposed to an MFL? Which historic flows are meant here (given that flows in 1971-1989 are considerably lower than subsequent flow).

p. 153 - Is the second management target meant to describe the situation at RM 9.2 or 9.7? This should be stated.

p. 155 - How might repairs to the Dam affect the calibration of flow? If there are major leaks now, this could also affect flow/salinity relationships.

p. 160 - The information discussed here cannot be found in Table 5.

Appendix A had figures missing.

Appendix B: Isn't this supposed to be a comparison of 2 interpretations of vegetation from 1940? This is not clear.

Appendix C. It is difficult to follow the analysis of Ds/Db presented in Table C-4 without the information presented in Tables 30-32, 36, and 39. The document would benefit if the information presented for red maple in Table 39 was presented for all species in the Appendix.

Appendix D

Appendix D describes the use of data from 1997 through 2000, yet some of the graphs begin in 1994 and others begin in 1996. It would probably be best to use the data from after the gaps were closed, as this added 0.7 miles to the channel.

SAS analyses need to be performed for stations 66 and 67.

All the Excel graphs should be deleted, since we know there are errors in the way Excel computes curve fits.

Table D-1 needs to be redone to reflect the appropriate dry season discharges derived from the SAS fits. The flow-averaging period that produces the best fit is probably the one to use (this varies from 3-d for all data to 9-d for after the closure of the gaps, which is more evidence that these relationships changed at that time). Station 65 produced the best fit on the day of observation, perhaps because it is closer to the Dam.

Once the graphs and Table D-1 have been updated, the text in this Appendix needs to be changed accordingly.

Appendix E.

I only had black and white copies of the figures and so had a lot of difficulty interpreting them.

I do not understand the paragraph on p. E-18 that describes Figures 12-15.

Appendix H states that the salinity data set was estimated based on flow relationships developed in Appendix D, but as far as I can tell these empirical relationships were not used.

Appendix N had figures missing.